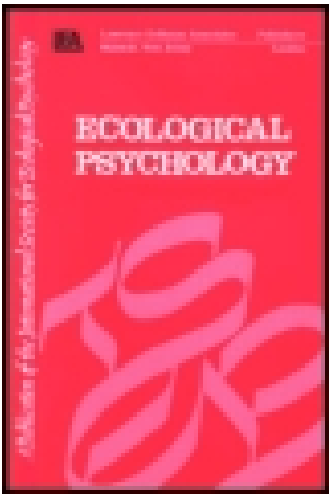

Ecological Psychology
Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/heco20

A Pragmatic Conception of Basic and Applied Research: Commentary on Hoffman and Deffenbacher (1993)

Kim J. Vicente

Published online: 17 Sep 2010.

ECOLOGICAL PSYCHOLOGY, 6(1), 65–81

COMMENTARY

A Pragmatic Conception of Basic and Applied Research: Commentary on Hoffman and Deffenbacher (1993)

Kim J. Vicente
Cognitive Engineering Laboratory
Department of Industrial Engineering
University of Toronto

Hoffman and Deffenbacher (1993) proposed a multidimensional framework that is intended to describe the relationship between basic and applied behavioral research. This is an extremely important topic in psychology and human factors, one that has a wide range of implications for many issues that are at the core of the scientific endeavor. Thus, it is worthwhile to critically examine the fundamental questions raised by Hoffman and Deffenbacher's article. Analyzing these same issues from the perspective of a different field, cognitive engineering, leads to an alternative conception of basic and applied research that is more parsimonious and more pragmatic than Hoffman and Deffenbacher's scheme. The key criterion for classifying research according to this alternative framework is the purpose of the research; the primary goal of basic research is to contribute to theoretical knowledge and the primary goal of applied research is to solve a specific problem. This framework is intended to promote a broad vision of psychology, and more importantly for human factors, a tighter coupling between basic research and applied problems.

Requests for reprints should be sent to Kim J. Vicente, Department of Industrial Engineering, University of Toronto, 4 Taddle Creek Road, Toronto, Ontario, Canada M5S 1A4. Email: benfica@ie.utoronto.ca.

> Putting my education to a practical test was a new education. I discovered that what I had known before did not work. I learned that when a science does not usefully apply to practical problems there is something wrong with the theory of the science. (Gibson, 1967/1982, p. 18)

Hoffman and Deffenbacher (1993) have made an important contribution by proposing one view of the relation between basic and applied psychological research. This is an issue of broad scope and fundamental importance, having far-reaching implications for virtually anyone involved in the construction and reconstruction of knowledge in the behavioral sciences. As a result, we can only gain from reflecting on the ideas that Hoffman and Deffenbacher have presented.

In tackling such a broad topic, one cannot possibly hope to propose a single viewpoint or "solution" with which everyone will agree. A more realistic aim would be to encourage discussion so that the relative advantages and disadvantages of different approaches can be pointed out. This commentary is proposed in this spirit. My goals are as follows: (a) to point out the importance of discussing the basic–applied distinction; (b) to critically examine the conception put forth by Hoffman and Deffenbacher, but from the perspective of cognitive engineering; (c) to point out some weaknesses in their scheme as viewed from this different perspective; and finally (d) to propose an alternate conception of basic and applied research that is more parsimonious and more pragmatically oriented.

This alternate conception should be judged according to the heuristic value it provides in conceptually organizing the various factors relating basic and applied research, and most important, according to the extent to which it encourages productive research that narrows the gap between the basic and applied worlds. Thus, the ideas presented in this commentary are utilitarian in nature and should not be interpreted as the "true" way to view basic and applied research.

WHY DOES IT MATTER?

A Personal Frame of Reference

My training is in engineering, not psychology, and thus, I come at these issues more from the applied end (albeit from within academe). Specifically, I work in the field known as *cognitive engineering*, which is concerned with human factors issues associated with complex sociotechnical systems (see Vicente & Rasmussen, 1990, for more details). Cognitive engineering takes a system-theory approach to these problems, placing a great deal of emphasis on identifying various classes of constraints that can shape goal-oriented behavior (Rasmussen, 1986). Consequently, cognitive engineering shares some conceptual ties with ecological psychology, which, as Lombardo (1987, pp. 327–329) pointed out, was also influenced by systems theory, a fact to which I will return later. Note that, because cognitive engineering differs from traditional human factors in significant ways (Vicente, 1990), other human factors engineers or psychologists may not share the opinions expressed here.

These facts have several implications that are tied to my motivations for participating in a discussion on the relationship between basic and applied research. The first point that needs to be recognized is that human factors is an inherently pragmatic discipline because the ultimate goal is to design better artifacts or working conditions. However, within the discipline, there are those (typically in academe) who are more concerned with basic research and others (typically in industry) who are more concerned with applied issues. Unfortunately, these two groups are rather segregated, resulting in a frustrating gap between basic research and applied problems (Flach, 1990; Rouse, 1985). As a result, the results of basic research are largely ignored by designers and therefore rarely have a significant impact on the applied problems that they are intended to address (see Vicente, Burns, & Pawlak, 1993, for a brief review). Thus, the field of human factors is facing a crisis that has motivated some (e.g., Flach, 1990; Vicente, 1990) to search for alternative approaches to overcome the current dismal state of affairs.

What implications does all this have for a discussion of the basic–applied relation? First and foremost, I firmly agree with Hoffman and Deffenbacher (1993) that there is a distinction to be made between basic and applied research. As I argue later, the goals of each are different as are the constraints under which they are conducted. This contrast between basic and applied is very clear in the field of human factors in which the aims, conditions, and skills required for basic and applied work are widely acknowledged to be quite different. As an example, basic research might be concerned with issues such as the effects of age on dual-task performance, whereas applied research might be directed at questions such as whether a new computer-based alarm system for a nuclear power plant control room results in improved performance compared to the previous alarm system. This difference is even reflected in graduate programs, with some universities being recognized for producing good academics and others for producing good industry people. Second, I firmly believe that viewing the relationship between basic and applied research from a different perspective can help the discipline of human factors overcome the existing gap between the basic and applied worlds. This is my primary motivation for engaging in this discussion. Thus, my aim is not to develop a conception of the basic–applied relation that captures the differences among existing research efforts (as Hoffman & Deffenbacher's aim seems to be), but rather to develop a conception that will promote a way of thinking and styles of research that can result in basic research that will have more to say about applied questions. This is of obvious relevance to human factors, but it also has advantages for psychology, as I will try to illustrate here.

My attempt to make my frame of reference explicit should evidence that the views expressed in this commentary are unavoidably value-laden. Subjectivity is common in almost all aspects of science (cf. National Academy of Sciences, 1989), but because the issues discussed here are so broad, the influence of one's frame of reference is without a doubt more pronounced than usual. This does not mean, however, that the classification of research as applied or basic is completely arbitrary and not worthwhile discussing. On the contrary, I think there are many important reasons for voicing alternate conceptions, despite the difficulty inherent in trying to reconcile views based on different value structures.

Broader Implications

Researchers' views of the basic–applied distinction affect the kind of research in which they think it is possible to engage, and also the kind of research they recognize as being sound. For example, some experimental psychologists seem to equate the term basic research with well-controlled laboratory studies (e.g., Banaji & Crowder, 1989, 1991). Thus, they would dismiss applied research that traded off control for relevance as being unscientific and, therefore, of little value. In the strong words of Banaji and Crowder (1991), "If you wish to do research that is useful (i.e., practical, functional) the *optimal* [italics added] path is controlled experimentation" (p. 79). Conversely, I have encountered human-factors engineers who think that it is impossible to do meaningful research (i.e., research in which the results will generalize to operational settings) in a laboratory setting, and because they equate basic research with laboratory studies, they wind up dismissing basic research as irrelevant to applied questions. These examples illustrate that one's view of the basic–applied distinction affects the kind of research one will conduct, praise, and perhaps even consider worthwhile publishing and funding. These decisions are at the core of the scientific endeavor.

Consequently, discussing how the relationship between basic and applied research can or should be viewed is worthwhile if for no other reason than that it forces each one of us to make our implicit assumptions and judgments on this topic explicit. By critically examining our suppositions, we can become aware of the limitations of our views. There is something to be gained in this exercise alone. To enjoy this benefit, we do not have to come to an agreement on how to view basic and applied research.

Although making our assumptions explicit is of value, I think there is another set of deeper benefits that can be derived from addressing these issues. In my opinion, the real benefit of trying to develop a conception of the basic–applied relation is that it forces one to consider a set of related issues—issues that I believe are, in the long run, more important than classifying research as basic or applied. Thus, in my view, developing a classification of basic–applied research is a means to an end, and not an end in itself.

For example, voicing alternative conceptions of the basic–applied distinction can make researchers aware of other ways of doing research that they have not considered. It also can make them aware of the value of certain types of research that they may choose not to conduct. The discussion can also allow us to determine the relative advantages and disadvantages of different ways of viewing basic and applied research, and even of that research itself. This will allow each one of us to make up our own mind about how to view the basic–applied distinction. For some, this reflective exercise can broaden their understanding of how their own research practices fit in with those of other researchers. For others, it may make them more accepting of the type of research conducted by other researchers, whose work is more basic or more applied than their own. For others still, making up their own mind may even lead to a change in the way they view research, and what issues they consider worthwhile to pursue. The opening quotation from Gibson indicates that reflectively examining the existing relationship between basic and applied issues played a significant role in Gibson's development of ecological psychology (see also Lombardo, 1987; Reed, 1988). Thus, examining the relationship between basic and applied research, as is argued in more detail later, can lead to broadening the scope of psychology. It also has important implications for human factors.

From my frame of reference, the single most important reason for trying to develop a coherent conception of the relationship between basic and applied research is that it can help to narrow the existing gap between basic research and applied problems. In practical terms, this would mean that the design of products would benefit more from insights gained from research, and that basic human factors research would be motivated more by significant applied problems. Such changes would affect all of us, not just those in the field of human factors. The increased influence of applied problems on basic research would make some research more socially responsible, which seems like a worthwhile endeavor given the current global economic climate. Furthermore, the increased influence of basic research on product design could lead to more usable and useful artifacts, and thereby to an improved quality of life (Nickerson, 1986).

In summary, I think Hoffman and Deffenbacher's (1993) discussion of the basic–applied distinction is extremely important and worthwhile to consider. Regardless of whether one eventually agrees with the conception they have proposed, important benefits can be accrued by considering a fundamental set of related issues that can affect how research is conducted and what research topics are addressed.

HOFFMAN AND DEFFENBACHER'S CONCEPTION

Hoffman and Deffenbacher begin with the *common conception* of the basic–applied relation. Briefly, the common conception views basic research as leading to "universal" principles and applied research as the mere application of these principles to specific applied problems. By implication, there is a unidirectional flow between the two, with basic research "feeding" applied research. Hoffman and Deffenbacher then proceed to present examples of research that are not cleanly captured by the common conception, thereby pointing out the weaknesses of that viewpoint. Their primary contribution is an alternative framework that is much more detailed, and therefore much more flexible, than the common conception. Their multidimensional framework consists of "outward-looking" dimensions that represent the relevance of research materials, methods, and settings to human experience outside the laboratory, and "inward-looking" dimensions that represent the relevance of experimental designs and hypotheses to theoretical topics. The framework is rounded off by dimensions representing the degree to which research results contribute to action and to knowledge. The stated purpose of this framework is to address questions about the relationships between basic and applied science.

There is much in Hoffman and Deffenbacher's formulation with which I concur. For example, I firmly agree that there need not be a strict dichotomy between basic and applied research. I also believe that ecological concepts such as representativeness have much to contribute in elucidating the relationships between basic and applied research. Furthermore, there is no doubt that, contrary to the common conception, applied research can indeed constrain and productively inform basic research. However, despite these points of agreement, several significant limitations in Hoffman and Deffenbacher's framework can be pinpointed if one views their ideas from the frame of reference I outlined earlier.

Limitations

This section outlines, from a cognitive engineering perspective, some of the limitations of Hoffman and Deffenbacher's framework. This analysis leads to an alternative vision of the basic–applied distinction that is more parsimonious, selective, and pragmatic. This vision consists of a unidimensional continuum of basic–applied research, in contrast not only with the hard and fast dichotomy of the common conception but also with the multidimensional scheme of Hoffman and Deffenbacher.

Be parsimonious. One of the disadvantages of Hoffman and Deffenbacher's framework is its lack of parsimony (as measured by the number of dimensions in their framework). As the authors candidly pointed out, "we have not overlooked the possibility that our 22-dimension analysis may be overkill" (Hoffman & Deffenbacher, 1993, p. 343). Although the common conception is limited, at least it has simplicity on its side. Is it possible to develop a more parsimonious framework while avoiding the problems with the common conception?

One suggestion that I made (which the authors acknowledge on p. 343) was to modify the categories used to describe the relations among an experiment's methods, tasks, and stimuli to the world outside of the research context (see their Table 3, p. 330). More specifically, it is possible to collapse the four dimensions of ecological validity, relevance, salience, and representativeness onto a single dimension defined by Brunswik's original concept of representativeness, because "representative experimental design also implies that the variables themselves should be sensitized to their biological relevance" (Brunswik, 1956, p. 12). Although the point is obviously debatable, it seems to me that this alternative formulation captures the essence of Hoffman and Deffenbacher's intentions in a more parsimonious fashion.

In justifying why they decided not to adopt this suggestion. Hoffman and Deffenbacher (1993) stated: "we have found, however, that four dimensions are useful both separately and in various combinations: Remote-sensing displays are ecologically valid, yet their *representativeness varies considerably and depends on the reference group*" (p. 344; italics added). It seems that Hoffman and Deffenbacher's interpretation of Brunswik's (1952, 1956) concept of representativeness is different from my own. As I understand the term, representativeness is always relative. That is, before one can define or evaluate representativeness, one must describe the target set (e.g., subject population, setting, task, work domain) to which one is interested in generalizing. As Brunswik (1952) pointed out:

> The study of functional organism–environment relationships would seem to require that . . . situational circumstances should be made to represent . . . conditions under which the organism has to function. This leads to what the writer has suggested to call the "representative design of experiments". . . . Any generalized statement of relationship requires specification of a "reference class" or "universe" from which the material is drawn. (p. 30)

Several examples can be cited to show how Hoffman and Deffenbacher's usage of *representativeness* differs from my own. For example, the criterion for their concept of ecological salience is an experiment that "can be judged to involve tasks or experiences that represent important things people do" (p. 330). One can ask: To whom is it important? Being Prime Minister of a country is an important task, but few people have such a job. Does that mean that an experiment requiring subjects to act as Prime Minister to control a simulated economy, for example, deserves the general label *ecologically salient*? A more homogeneous definition of the target population could lead to a more precise evaluation of an experiment's representativeness. Another instance of not defining the target set for generalization can be found in Hoffman and Deffenbacher's discussion of an experiment by Chechile, Eggleston, Fleishman, and Sasseville (1989) that required subjects to fly simulated missions on a computer. They stated: "While the displays and tasks may be judged to possess some ecological validity, the simulator situation seems low in ecological representativeness (few people are fighter pilots)" (p. 47). In my view of representativeness, one could restate this claim by saying that the experiment was somewhat representative, but only if one is interested in generalizing to the population of fighter pilots. This revised statement seems to be more precise than the original, and it requires only a single dimension instead of four.

This practice of not defining the target set for generalization explains why Hoffman and Deffenbacher feel the need for four dimensions to represent ecological representativeness. In the absence of an explicit definition of the target set, additional degrees of freedom are required to account for the fact that some situations are representative for some people but not for others. Using the single relational dimension of representativeness, as defined by Brunswik (1952), instead of the four dimensions in Table 3, would allow one to make more precise claims in a more parsimonious fashion.

A second way to modify Hoffman and Deffenbacher's scheme to make it more parsimonious would be to reconsider the dimensions listed in Tables 4 and 6 (pp. 331 and 336, respectively). These tables are intended to represent the relationship between an experiment's hypotheses and methods, respectively, and its epistemological context. However, examination of the tables reveals that the authors operationalize this as how closely an experiment conforms to the standards defined by "normal science," in Kuhn's (1970) sense (the authors themselves do not use this terminology). Dimensions are defined by relativistic phrases such as "available theories" and "accepted research methodology." But is the extent to which an experiment conforms to fashionable paradigms of the time really pertinent to the basic–applied distinction? An atheoretical, "quick-and-dirty" applied experiment (e.g., which is better, Design A or Design B?) would rank low on the criteria listed in Tables 4 and 6 of their article, as would an experiment that turned out to be the seed of a new paradigm for basic research. Lumping these two cases together is a significant limitation. Thus, the question of whether an experiment can be characterized as "normal science" seems to me to be orthogonal to the question of whether that research is basic or applied. This part of Hoffman and Deffenbacher's conception can therefore be critiqued for lack of parsimony in the sense that they introduce dimensions that really cannot be consistently used to describe the distinctions they are trying to capture. Given this argument, it follows that the dimensions in Tables 4 and 6 can be omitted from the framework.

Be selective. Another tactic for improving upon Hoffman and Deffenbacher's framework while being more parsimonious is to be more selective. That is, rather than try to accommodate all research that has been conducted, one could focus on including only research that is conducted according to certain prescriptive standards. Perhaps some would object to this tactic because it is admittedly based on an engineer's bias. (Design is always based on normative goals.) However, this tactic may not seem so foreign if one considers that, implicitly or explicitly, every researcher holds a prescriptive norm for evaluating research. Therefore, it might not be very productive to lump all research ("good" and "bad") into a single category, about which claims are to be made as a whole. Why not make the prescriptive criteria guiding evaluation of research explicit and use them as a basis for selecting the research to be included in a conception of basic–applied research, and even to define that conception itself?

What would such a selective/prescriptive approach look like? To be prescriptive, one needs a referent, and in this case, as in others, there may many alternatives from which to choose. Given the ties between cognitive engineering and ecological psychology alluded to earlier, it makes sense to adopt a set of basic criteria that characterize research in ecological psychology to define what one might mean by "being prescriptive." Two such criteria (Vicente, 1990), and the implications they have for research, are described next.

First, in ecological psychology the organism–environment system is the fundamental unit of analysis. As a result, the context in which behavior takes place plays a very important role because behavior is often influenced by environmental constraints. Second, ecological research is based on the representative design of experiments (Brunswik, 1956). As mentioned earlier, this implies that research should be based on biologically or socially relevant phenomena, and that the conditions governing experimentation should be representative of the target situation to which one is interested in generalizing. As a result, laboratory studies can and should have many similarities to situations outside the laboratory. These beliefs allow us to be selective in the sense that they provide a basis for developing a conception of basic–applied research that only accommodates research that is consistent with these beliefs. On the one hand, such a framework would not accommodate previous research that is poorly conceived (according to these criteria). On the other hand, such a framework would accommodate styles of research that perhaps have tended to be ignored in the past (e.g., conducting basic research in more realistic settings outside the laboratory), thereby opening up relatively unexplored but promising degrees of freedom for research.

Consider how these criteria provide a way of avoiding the rut of previous practices. Chapanis's (1988) complaint that basic research is not generalizable, for instance, is an excellent example of a failure to be selective. More specifically, his argument is predicated on a definition of basic research that is totally at odds with the criteria of ecological psychology:

> Subjects are solicited, or conscripted, from some conveniently accessible population. They are brought into a laboratory and are confronted with an experimental

> setup—a particular piece or arrangement of apparatus. The apparatus may be something that has been used before and so, in the interests of economy, is made to serve again, or it may be an apparatus fashioned according to the experimenter's predilections. Carefully controlled stimuli with defined characteristics are presented to the subject. Often the stimuli are things a subject may never or rarely experience in a lifetime, such as spectrally pure lights, pure tones, nonsense syllables, or nonsense forms. From a large number of possible dependent variables the experimenter selects one or a few that, in his or her opinion, are most likely to yield meaningful results. The subject is instructed about what is to be done, is given a few trials to become familiar with the apparatus and procedures, is tested for perhaps an hour or so, and is then dismissed. (Chapanis, 1988, pp. 254–255)

It seems clear from the context surrounding this quote that Chapanis offers a serious characterization of basic research, not one that is tongue-in-cheek. And although there is obviously some truth to Chapanis's comments, they are extremely misleading as stated. What Chapanis failed to point out is that this is a characterization of how basic research has tended to be carried out (in the information processing paradigm), not how it can be carried out, or should be carried out. It is no surprise that Chapanis concluded that basic research is not necessarily more generalizable than applied research!

Hoffman and Deffenbacher adopted a similar stance in several places. For instance, one of their criticisms of the common conception is that basic research can lead to specific knowledge instead of general knowledge (p. 320; see also p. 337). But surely this is the fault of the researchers who conducted that research, not of the common conception. I think most people would agree that basic research is intended (in some sense) to be broad in scope. If specific studies do not live up to that objective, it is because they have been poorly conducted (often due to unrepresentative experimental designs). I do not see the value in developing a framework that accommodates such research, especially if it means adding more dimensions to fit these poorly conceived studies into the framework.

Another example illustrating that Hoffman and Deffenbacher's dimensions are based on how research has tended to be conducted in the past can be found on p. 330. Hoffman and Deffenbacher stated that research is applied if it "seems relevant to things people actually do, things they do that are salient, things they do that are important, or things they do often" (p. 330). Conversely, they later say that research can be considered to be basic if it uses artificial stimuli, if it involves tasks that rarely occur outside the laboratory or that people rarely do or that are not important (see also the top half of their Table 1, p. 319). Given the criteria associated with ecological psychology, this classification of basic or applied research is unacceptable (Hammond, 1993). The properties that are listed as belonging to applied research are actually those that one would find in research that attempts to be representative, that is, say something about human behavior outside the confines of the lab. Such research can be either basic or applied. The properties that are listed as belonging to basic research are those that characterize "basic" research conducted according to the information processing paradigm that assumes that it is possible to extract "universal" psychological principles by observing human behavior in context-free settings. As with Chapanis's characterization, I do not see the value in using such criteria to define the basic–applied distinction. One should look forward to how research can be conducted rather than base one's conceptions of basic and applied research on traditional practices that are, from an ecological perspective, severely flawed.

As a step in that direction, I think there are good reasons for arguing that the setting and problem area adopted for research should not be used to uniquely identify research as being applied or basic. In the case of setting, Hoffman and Deffenbacher suggested that more "realistic" settings are associated with applied research. However, there is no reason why one cannot ask basic, fundamental questions in a realistic, complex setting (Hammond, 1989). For instance, if one were interested in understanding the effects of complexity and time pressure on human problem solving and action, what better place than an airplane cockpit or nuclear power plant control room, or better yet both, to test for generalizability? The same case can be made for the problem area selected for research. Hoffman and Deffenbacher actually pointed out that "applied" problems can stimulate basic science (e.g., studies of very long-term recall of life events can have implications for theories of remembering). I agree completely, but I do not see why such research should be characterized as applied just because it is not using a context-free topic for the conduct of research. In other words, there is no reason why one cannot conduct basic research within the context of socially or biologically relevant problems. In fact, there are very good reasons for doing so (see Hammond, 1989; Vicente & Rasmussen, 1990). If one accepts these arguments, then Hoffman and Deffenbacher's Table 5 (p. 336), which describes how closely an experiment's hypotheses relate to the world outside of the lab, is not needed to classify the basic–applied distinction because both basic and applied research can address socially or biologically relevant questions.

In summary, I have argued that one should not develop a classification of the basic–applied distinction that is rooted in traditional research practices. The fundamental beliefs that characterize ecological psychology allow us to be more selective, thereby developing a prescriptive scheme—one that describes how basic and applied research can and should be practiced. Doing so simplifies Hoffman and Deffenbacher's framework because the setting and problem area are no longer needed to determine whether research is basic or applied.

Be pragmatic. There is another cornerstone of ecological psychology that can also play a prominent role in developing an alternate conception of basic–applied research. I am referring to the importance of specifying goals in characterizing and understanding behavior. Given the strong pragmatic foundations of ecological psychology (cf. Gibson, 1979/1986; Lombardo, 1987; Reed, 1988), it seems to me that the primary characteristic that should be used to distinguish basic from applied research is the purpose behind that research. This is captured in Hoffman and Deffenbacher's Table 7 (p. 338), which categorizes research as trying to contribute to action or to knowledge. They subdivide this distinction into six categories, representing utility, novelty, and generality along both ecological and epistemological dimensions. However, for the sake of parsimony, it is possible to represent the essence of the distinction along a single continuum. On the one end, there is basic research that attempts to make a broad contribution to the principled understanding of human behavior. At the other end, there is applied research that attempts to answer a very specific question in a very limited context so as to select between a set of alternatives for action. This latter type of research is prevalent in the human-factors community because designers are often faced with highly context-specific questions that are not answered by the existing research base (Rouse, 1985). When such a question is important to the success of a project, designers may conduct "quick-and-dirty" experiments to obtain an answer. When the question is not so important, they may try to obtain an answer by methods that would not even qualify as research (e.g., asking a colleague, looking up an old design, etc.).

There is an obvious difference in scope along this continuum, with research on the applied end being much more narrow. However, there is also a difference in terms of the general types of questions being asked. To simplify matters, the basic researcher is interested in understanding *why* things are the way they are, whereas the applied person is primarily interested in understanding *what* differences exist between specific alternatives that are available to them. To take an example from the human-factors domain, the goal of basic research could be to develop a framework that allows one to design better computer interfaces. Because such a framework would be intended to have broad applicability, it would have to be theoretically motivated to explain how changes in characteristics of the interface can affect human performance under various conditions (e.g., task demands). In contrast, the goal of applied research in the same area could be merely to determine which of two interfaces leads to better performance for a specific context and for a specific population of human operators. There is no need to understand why, merely to determine which is the better interface. This contrast, which can be viewed as the difference between acquiring broad, theoretical understanding and solving a specific problem, reflects the difference in purpose behind basic and applied research, as defined here.

A PARSIMONIOUS, PRAGMATIC CONCEPTION

So what are we left with if we adopt all of the changes recommended in the previous section? The result, I believe, is a more parsimonious and more pragmatic conception of the basic–applied distinction. This alternative framework is defined primarily by a single dimension representing the purpose of the research. Applied research seeks to answer very specific questions with very little concern for theoretical understanding. Basic research, however, seeks to answer questions of broader significance in a manner that leads to a principled understanding of the phenomenon being investigated. No direction of flow needs to be specified because, as mentioned earlier, either type of research can inform and influence the other.

Interestingly, there is a rather unusual piece of "evidence" that I have recently come across that can be cited in indirect support of the framework I am proposing. This evidence is taken from a recently formulated mission statement of Risø National Laboratory in Roskilde, Denmark—a prominent laboratory that is involved in multidisciplinary research, some of which is behavioral in nature—which now appears on the outside back cover of their technical report series. Risø's stated objective is to further technological development in energy, environment, and materials. The results of Risø's research have been widely applied in agriculture, industry, and public services. The most relevant part of the mission statement deals with the research profile of the laboratory. There, it states that the emphasis is on "long-term and strategic research providing a solid scientific foundation for the technological development of society." Figure 1 is an adaptation of a figure that is used by Risø to illustrate graphically the emphasis of its research profile (the graph is qualitative in nature and is not based on actual data).

The abscissa of the figure is defined by two poles describing the purpose of the research, knowledge-oriented and market-oriented. Basic research falls at the

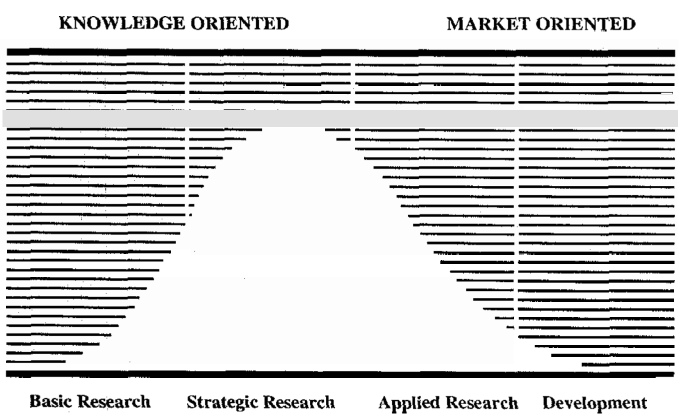

FIGURE 1 A unidimensional scheme for classifying basic and applied research recently adopted by Risø National Laboratory. The scheme is similar to that proposed in this commentary in that the primary distinction is pragmatically oriented, distinguishing between knowledge-oriented and market-oriented activities.

extreme of knowledge-oriented research. Applied research falls on the market-oriented end of the continuum. Interestingly, however, it is not at the end of that part of the continuum. Development serves as the pole of market-oriented activities. Here, the goal is pragmatic to the point that research is no longer needed or conducted. The goal of development is to create a product that instantiates the ideas generated from basic, strategic, and applied research. The similarity between the framework that Risø chose to describe its research profile and the framework proposed here is striking.

What about the concept of representativeness; where does that fit in? Because it is concerned with very narrow contexts, applied research tends to be conducted under more representative conditions than basic research. The primary reason for this is that human activities tend to take place in rich, complex settings, but it is very difficult to conduct defensible research in the presence of such complexity (Cook & Campbell, 1979). As a result, basic research tends to trade off representativeness for experimental control. There is a lesser need to do this with applied research because the emphasis is not on theoretical understanding, thereby reducing the need for control.

THE REAL CHALLENGE: COMBATTING HAMMOND'S LAW

Because the vast majority of the readers of this journal are more likely to be concerned with basic rather than applied research, it is worthwhile delving into the constraints on this type of research in a bit more detail. As defined here, the goal of basic research is to produce knowledge of broad scope that explains why people behave the way they do. The dual goals of broad generalizability and defensibility, however, create a conflict. Hammond (1989) captured this relation in what he calls the law of the laboratory: "Rigor is inversely related to representation of complexity" (p. 2). As Hammond (1993) pointed out, traditional psychological research certainly has taken an extreme approach to this conflict, emphasizing control to the point where generalizability to meaningful human activities is severely compromised (see also discussion, earlier). Is it possible to deal with this conflict in a less reckless manner?

Hammond (1993) proposed one approach to this problem, based on the ideas of Brunswik (1956). As a complement, the approach that human-factors researchers (Sheridan & Hennessy, 1984; Woods, 1984) have suggested is to incorporate a multiplicity of methods with different characteristics to overcome the conflict just described. The basic idea is to conduct several converging investigations with varying degrees of representativeness. So for example, field studies can be used to observe behavior *in situ*, thereby achieving a large degree of representativeness (but virtually no control). Such studies could be used to identify phenomena that are important and therefore worthwhile studying under more controlled conditions. Then, laboratory studies can be conducted under more controlled conditions to try and develop causal explanations for the phenomena observed in the field. In human-factors research, the pragmatic value of these causal explanations then can be tested again in the field in which the impact of the theoretically motivated manipulations can be evaluated in the presence of a wide range of factors that had been controlled for or eliminated in the laboratory. This approach allows one to achieve generalizability and defensibility in the same research program, thereby cutting through the conflict described earlier.

Although this approach has been advocated by human-factors researchers, it easily transfers to psychology as illustrated by the work of Vicente and Brewer (1993). Their research was prompted by the fact that de Groot's (1965) seminal work on the relationship between memory and chess expertise had been repeatedly cited erroneously in the scientific literature. Their initial effort was descriptive and naturalistic, searching the literature for patterns that might help give clues as to the cause of the errors. A close examination of the errors suggested that they may have been due to the biases of reconstructive remembering (Bartlett, 1932). To test the plausibility of this hypothesis, Vicente and Brewer asked a number of cognitive psychology professors to recall, unaided, the details of de Groot's well-known experiment. The results showed that the professors exhibited many of the errors found in the literature. To test the reconstructive remembering hypothesis more rigorously, some controlled memory experiments were conducted in the laboratory. These studies confirmed that the reconstructive biases of remembering could be one of the causes of the errors observed in the literature.

Vicente and Brewer's investigation shows that the tactic of adopting a number of methods varying in representativeness and control (descriptive bibliographic analysis, open-ended questionnaire, and controlled experiments) allows one to conduct research that is both defensible and generalizable to meaningful contexts outside of the laboratory (see Lave, 1988, for another similar example). Some might argue that this research tactic is self-evident and should be familiar to any good ecological psychologist. Although there may be some merit to this criticism, it is also true that few studies in this journal, for instance, adopt this approach. As a result, it seems worthwhile to point out the advantages of such a strategy.

CONCLUSION

Hoffman and Deffenbacher (1993) have taken on the difficult task of proposing a framework that endeavors to account for the relationship between basic and applied research. In my view, the value in considering this issue is that it causes one to reflect on a host of other related questions that I think are more important in the sense that they can affect what research is conducted and how it is conducted, rather than just how it should be labeled.

This commentary is a reflection from the viewpoint of cognitive engineering, a discipline in which the relation between basic and applied research is of fundamental importance. An alternative vision of the basic–applied distinction has been proposed, consisting of a single dimension representing the purpose of the research, to contribute to knowledge or to solve a specific problem. It was also pointed out that scientific activity along this dimension is subject to Hammond's law of the laboratory, that is, the trade-off between rigor (or control) and representativeness. The challenge for researchers is to develop research programs that cut through this conflict. Doing so will result in a science of human behavior that has something to say about activities outside the laboratory. As the opening quotation makes clear, this was an important concern of Gibson. In addition to leading to a richer vision of psychology, combatting Hammond's law will also help to reduce the deplorable gap that currently exists between basic research and applied problems in human-factors engineering.

ACKNOWLEDGMENTS

I thank Ted Cochran and JoAnne Wang for providing useful comments on earlier drafts. Also, I am deeply indebted to Bill Mace for providing me with the opportunity to write this commentary, and for providing exceptionally thorough and constructive criticisms, thereby forcing me to make my own suppositions more explicit.

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